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Patrick Jöckel
Editor,
Atmospheric Chemistry and Physics

Dear Patrick,

Re: Response to comments on 'Inclusion of mountain wave-induced cooling for the formation of PSCs over the Antarctic Peninsula in a chemistry-climate model' by A. Orr et al.

Thank you for the two reviews of the above manuscript. We are tremendously grateful for the Reviewer's careful and insightful comments, which have contributed significantly to the improvement of the original manuscript. We have worked hard to incorporate their comments into the revised manuscript, and hope you find the changes satisfactorily.

Yours Sincerely,

Andrew Orr

Anonymous Referee #2

General comments

1) *From the abstract, the introduction and the model description the paper promises to resolve a longstanding problem in chemistry climate models, most results are however presented just for one gridbox and for an example at an altitude and time that is not relevant for the ozone hole. In Figure 5 are more interesting cases which were not addressed.*

Yes, these concerns have been clarified/addressed in the revised manuscript.

Firstly, the abstract and introduction have been revised to make clear that the paper does not resolve 'a longstanding problem in chemistry climate models' (i.e. the realistic representation of mountain wave-induced PSCs and their attendant ozone-loss chemistry), but rather is a step towards this. This is achieved by emphasising that the chemistry-climate model is only used to perform sensitivity studies focused on the representation of regional PSCs for the Antarctic Peninsula. To this end, the final couple of sentence in the abstract have been revised to read: '***The parameterisation was used to include the simulation of mountain wave-induced PSCs in the global chemistry-climate configuration of the UM. A subsequent sensitivity study demonstrated that regional PSCs increased by up to 50% during July over the Antarctic Peninsula following the inclusion of the local mountain wave-induced cooling phase.***' Moreover, two additional sentences have been added to the introduction section which read: '***However, we only evaluate the sensitivity of PSC simulation in the chemistry-climate model to the inclusion of the mountain wave-induced temperature fluctuations for one regional example, the Antarctic Peninsula. (The contribution of the scheme to global PSCs and ozone chemistry will be fully assessed in a subsequent manuscript.) This is because the main purpose of this study is to assess the ability of the parameterisation to simulate stratospheric temperature fluctuations, which is achieved by using case studies of AIRS measurements to validate high horizontal resolution simulations (using the regional mesoscale configuration of the UM) of mountain wave-induced stratospheric temperature fluctuations above the Antarctic Peninsula (section 3)***'.

Secondly, we have addressed the reviewers concern that the results of Figs. 5, 7 and 8 are just for a single grid point (located at -70°N and -63.75°E). This is a fair comment, as although Fig. 3 shows that this point is representative of the CS3 mountain wave event, it is less representative for CS1 and not at all for CS2. In the revised manuscript we therefore select points individually for each of the three case studies which coincide with the location of the respective mountain wave event. These are located at (67.5°S, 63.75°W) for CS1, (65.0°S, 60.0°W) for CS2, and (70.0°S, 63.75°W) for CS3. Following this, it was evident that while the revised Fig. 5 suggests that the temperature fluctuations computed by the parameterisation scheme are in good agreement with the mesoscale results for CS1 and CS3, the agreement for CS2 is poor. An explanation for the poor agreement for CS2 is that the surface winds in CS2 were significantly different in the climate model compared to the mesoscale model, suggesting poor skill in capturing surface winds by the climate model which impacted the parameterisation response. Section 4 has been modified substantially to reflect these results. Moreover, the sentence in the abstract which stated in the original manuscript that the parameterised temperature fluctuations '***are in excellent***

agreement with the mesoscale configuration responses*'** has been revised to say they '**are *in good agreement with the mesoscale configuration responses for two of the three case studies'.

Thirdly, we disagree with the comment that 'most results are however .... for an example at an altitude and time that is not relevant for the ozone hole'. Here, the reviewer is referring to Figs. 9, 10 and 11 which were for July at an altitude of 21 km. However, we accept the relevance of this month and altitude to ozone depletion was not made clear in the original manuscript. To remedy this, the beginning of the third paragraph of the introduction has been revised to say: '***The role of PSC particles in polar ozone chemistry is generally well understood. In the winter when there is not enough sunlight in the polar stratosphere to initiate photochemistry, the conversion of reservoir chlorine molecules into chlorine gas takes place on the surface of PSCs. In the spring when the polar stratosphere becomes sunlit, ultra-violet radiation splits the chlorine gas molecules into chlorine atoms, which take part in reactions which destroy ozone (Solomon, 1999).***' Moreover, an additional sentence has been added to the final paragraph of the introduction which reads: '***Note also that the availability of sunlight at the Peninsula during mid-winter to initiate photochemistry means that ozone depletion is substantial over this region from mid-winter onwards (Roscoe et al., 1997).***' Finally, the reviewer should note that, despite the slower pace of ozone loss in the edge region of the vortex (where the Antarctic Peninsula lies), ozone amounts there regularly fall below 120 DU, very comparable to the 100 DU amounts seen in the vortex core, and that the edge region of the vortex contains about half the area of the ozone hole (Roscoe et al. 2012). Hence conditions in winter over the Antarctic Peninsula are of extreme relevance and interest to the ozone hole.

Roscoe, H. K., M. Trainic, W. Feng, M.P. Chipperfield, E.F. Shuckburgh, "The existence of the edge region of the Antarctic stratospheric vortex", J. Geophys. Res. 117, D04301, doi:10.1029/2011JD015940 (2012).

Fourthly, the reviewer makes the comment 'in Figure 5 are more interesting cases which were not addressed'. We reiterate that Fig. 5 (and more generally the three case studies) are purely used to validate/assess the temperature fluctuations computed by the parameterisation scheme. The investigation never had any intention of examining or simulating PSCs for these case studies.

2) *What happens at other gridboxes shown in Figure 11 in the light of the problems shown in Figure 6 (and page 18290, lines 25ff)? Can the approach be generalized also for the Arctic? Is the mountain wave parameterization used only for temperature or also winds and advection of chemical species? Here a lot of clarifications are necessary. To be acceptable major revision is needed.*

Yes, these concerns have been clarified/addressed in the revised manuscript.

Firstly, with regard to what happens to other grid boxes, please see our reply to general comments #1 (above).

Secondly, Fig. 6 has been revised to also show the parameterised cooling phase ΔT^{-}_{SSO}, which is the field which is passed/coupled to the PSC scheme of the chemistry-climate model.

Thirdly, the parameterisation scheme is (globally) implemented in the climate and chemistry-climate configurations of the UM, and so can be used for the Arctic. This has been made clear in the revised manuscript. For example, we have added an additional sentence to the final paragraph of section 6 which reads: '***Further future work will also involve evaluating and improving the representation of PSC formation mechanisms in the chemistry-climate model via comparison with MIPAS (Michelson Interferometer for Passive Atmospheric Sounding) PSC observations (Spang et al., 2012), resulting in improved modelling and more reliable projections of both Antarctic ozone hole recovery and Arctic ozone.***'

Fourthly, the parameterisation is only for temperature, and not for winds or advection of chemical species. To make this clear an additional paragraph has been added to the end of section 2.1 which reads: '***Note that all of the configurations of the UM parameterise the vertical divergence of mountain wave-induced momentum flux (i.e. orographic gravity wave drag), which influences the atmospheric circulation. This is dealt with by the orographic gravity wave drag scheme of Webster et al. (2003), which should not be confused with the mountain wave-induced temperature fluctuation scheme of Dean et al. (2007), described below.*** '

Specific comments

1) *Page 18278, line 24: This important finding is misleading here since it is confined to a special case not relevant for polar chemistry (see section 5).*

Yes, we have revised this line (situated in the opening paragraph of section 1), which now reads: '***Gravity waves generated by stratified flow passing over orography (mountain waves) that propagate into the stratosphere can play a role in the formation of polar stratospheric clouds (PSCs).***'

2) *Page 18279, line 15: It would be interesting to see results on this.*

The primary objective of this work is to investigate/validate the representation of mountain wave-induced temperature fluctuations by the parameterisation scheme. See reply to major comment #1 (above).

3) *Page 18283, line 24: On the Peninsula or global?*

Yes, the parameterisation scheme is implemented globally. To make this clear, we have revised one of the sentences in section 2.2 which describes the parameterisation to read: '***The parameterisation scheme is (globally) implemented in the climate and chemistry-climate configurations of the UM.***'

4) *Page 18285, line 7: Are these parameters specific for the Antarctic Peninsula or are they used for other mountain ranges too in the climate model?*

Yes, these parameters are specific for the Antarctic Peninsula. To make this clear, we have revised one of the sentences in section 2.2 to read: '***Following an initial sensitivity study (specific to the Antarctic Peninsula) to optimise the performance of the scheme, their values were set to*** $n_\sigma = 3$, $F_{sat} = 2$ ***and*** $F_c = 4$ ***(not shown).*** '

5) *Page 18286, line 18: Forecast from which model? A weather forecast model as on page 18283 (line 5) or the nudged climate model? Please clarify here, not one page later.*

Yes, this has been clarified. One of the sentences in section 2.4 has been revised to read: '***These events were simulated by running the nested mesoscale model for a 48 h period, driven by output from the global model initialised on 5 August 2011 at 12:00 UTC for CS1, 1 August 2010 at 00:00 UTC for CS2, and 13 July 2010 at 00:00 UTC for CS3.***' The use of the terms 'nested mesoscale model' and 'global model' are now clearly explained in section 2.1, which now states: '***The mesoscale model is nested within a global version of the model at a horizontal resolution of N512 (1024 × 769 grid points, or 0.352° × 0.234°) and 70 vertical levels (going up to 80 km) that is used (following a 3 h spin-up) to initiate and provide boundary conditions for the simulation.***' Additionally, section 2.1 now states (with the addition of one extra sentence): '***The UM is a numerical modelling system based on non-hydrostatic dynamics which can be run with varying configurations, including for this study as a regional mesoscale model, a global climate model, and a global chemistry-climate model. Hereafter, these models are referred to as the mesoscale, climate, and chemistry-climate models, respectively***.'

6) *Page 18287, line 4: This paragraph should go at least partially to section 3, here it interrupts the flow.*

We disagree with this suggestion. Section 3 is a results section, and as such should not contain any methodology.

7) *Page 18288, line 12: Where? Peninsula only?*

Yes, we have clarified this. In the revised manuscript it is now clearly defined in section 2.1 that the chemistry-climate model is a global model (see reply to specific comments #5), but that we are focusing on one regional example, the Antarctic Peninsula. However, to avoid confusion we have added an additional sentence to section 2.4 which states '***For this part of the study we again concentrate on results for the Antarctic Peninsula, focusing on the month of July.***' See also reply to general comments #1 (above).

8) *Page 18290, line 18: More quantitative information would be good here.*

Figure 1 (below) demonstrates clearly that the climate model at N96 resolution completely fails to resolve any temperature fluctuations. To clarify this in the revised manuscript we now state '***Note that the climate model simulations of the three case studies were repeated at a higher N96 resolution (192 × 145 grid points, or 1.875° × 1.25°), which also completely failed to resolve any temperature fluctuations over the Peninsula (not shown).***'

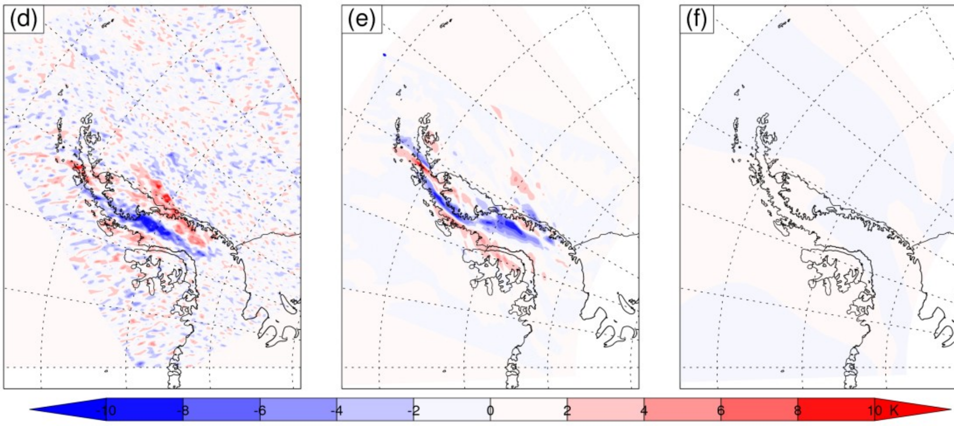

Figure 1: Measured (d), mesoscale model (e), and climate model (f) estimates of the temperature perturbation (K) at 21 km at the time of the CS1 mountain wave event. Measured data corresponds to the 666.5 cm^{-1} AIRS channel. Mesoscale model data corresponds to the 4-km-resolution UM regional mesoscale model. Climate model data corresponds to the N96 (1.875° × 1.25°) UM global climate model.

9) *Page 18291, line 5f: In N96 the smoothing should not be so severe. Can the N96 results be shown too? Line 20: I don't believe that.*

Firstly, the optimisation of the mountain wave parameterisation scheme for the Antarctic Peninsula was only done for N48 resolution. We therefore do not have equivalent results at N96 resolution which we can show.

Secondly, we have removed 'line 20' which stated '***If the mesoscale model response would be spatially averaged the amplitudes would be in good agreement.***' The analysis presented in Fig. 5 of the manuscript (i.e. assessing the derived temperature fluctuation for different climate model grid boxes around the Antarctic Peninsula) is a more quantitative comparison than simply averaging over a long transect as the negative (cooling) and positive (warming) phases largely cancel each other out. Therefore whether the amplitudes were in good agreement when averaged over a long transect is not important. We have added an additional sentence in section 4 which includes words to this affect.

10) *Page 18291, line 27ff: The selected time and altitude is not interesting for the ozone hole formation even if there are some effects on PSCs. Is it selected because of AIRS (Figure 1)? It is also odd that in Figures 5 and 6 there is a warming at this altitude due to the parameterization. An analysis in August and September at 18km would be much more relevant for the ozone hole formation. It is odd that the PSCs increase upstream of the mountain range (Figs 9 and 11).*

See reply to general comments #1 (above).

11) *Page 18293, line 11: In the example most of the Peninsula is still in polar night so ozone loss and radiative heating effects should be marginal (please check and quantify). Line 22: Why is there no effect in August to October? Here (re-)formation of PSCs at the Peninsula would be important. More details please.*

Yes, this has been clarified.

We have removed the line '***Note that significant differences in PSC surface area density were also evident in June, but not in August and September (not shown).***' from the revised manuscript, which was a throwaway line at best. We stress again that the aim of this study was to only show the sensitivity of PSCs to the inclusion of parameterised mountain wave-induced cooling, and this is demonstrated adequately for the July example.

Nevertheless, the reason why there is a much smaller effect in August and September could be because forming PSCs earlier in the winter dehydrates the stratosphere, making it harder for PSCs to form in late-winter and early-spring. This will be the focus of future investigation.

12) *Page 18295, line 28ff: The beginning of this paragraph would better fit into the introduction. The end needs clarifications, e.g. where exactly the parameterization is applied in the chemistry-climate model.*

We disagree that the beginning of this paragraph would better fit into the introduction. Here, we discuss that other biases could affect the ability of chemistry-climate models to realistically represent PSCs, which is entirely suitable for the discussion section. We further don't believe that the end of this paragraph needs further clarification. For example, it is clearly explained in the methodology section that the parameterisation is implemented in a global chemistry-climate model and that the wave-induced cold phases is coupled to the PSC scheme. Moreover, we reiterate this in section 6 by saying: '***Subsequently, we assessed and characterised the localised impact of the parameterised temperature fluctuations in a comprehensive chemistry-climate model. It was found that adding the wave-induced cooling phase to the resolved temperature ....***'

13) *Page 18305, figure caption: What is wind magnitude? The figure is difficult to read.*

Yes, this change has been made. This figure has been revised so as to make it more readable. The term 'wind magnitude' in the figure caption has been changed to 'wind speed'.

Technical corrections

1) *Page 18288, line 25: Bad wording, do you mean sea ice fraction, thickness or what?*

Yes, this has been clarified. We have replaced 'sea ice concentration' with 'sea ice fraction'.

2) *Page 18290, line 11: typo*

We couldn't find a typo on this line. The sentence states '***However, at all altitudes*** ΔT_{SSO} ***lies within the spread of*** ΔT_{MES} ***(which reaches ± 15 K in the lower***

stratosphere), suggesting that the parameterised temperature fluctuations are representative of the range of mesoscale model responses.'

3) *Page 18294, line 29: There is a word missing or too much in the sentence.*

Yes, this has been clarified. To make this sentence easier to read we have shortened it. It now simply says '***However, the comparison also showed that the parameterisation cannot represent the upstream tilt of the phase lines with height, due to it representing the Peninsula by a series of independent sub-grid scale ridges which each launch a mountain wave vertically through the column of air above.***'

Anonymous Referee #3

General comments

1) *Changes in PSC area in the model are discussed throughout the manuscript in a quantitative manner. However, the decision to include only the cooling phase of the clouds renders these numbers a bit dubious. While there is clear justification for including only cooling effects in the chemistry calculation, this is not at all clear for the PSC area calculation where exposure to both the cooling and warming phases is likely to be important. This is explained in context in the comments below. The suggestion is to discuss these numbers more qualitatively due to the uncertainty associated with the assumptions in the method.*

In the results sections of the manuscript PSC surface area density is only shown in Figure 11, which is solely discussed in the final paragraph of section 5 in broad terms such as '***increases of more than 50% over the northern tip of the Antarctic Peninsula, and at least 30% over the Bellingshausen Sea***'. These findings are reiterated in the abstract and discussed in the Summary and discussion section. We argue that a limited quantitative discussion of the numbers is required (as we have done) in order to detail the result that the PSC scheme is surprisingly sensitive to inclusion of the mountain wave-induced cooling phase. Indeed, the sentence in the abstract ('***A subsequent sensitivity study demonstrated that regional PSCs increased by up to 50% during July over the Antarctic Peninsula following the inclusion of the local mountain wave-induced cooling phase.***') leaves the reader in no doubt that the results were obtained by including only the cooling phase.

The reviewer is correct that we had not included the caveat that neglect of the warming phase, while done for justified reasons, possibly overestimates the impacts of the mountain wave parameterisation and its influence on PSC surface area density. To remedy this, the revised manuscript now includes the following changes. Firstly, the methodology (section 2.4) now clearly justifies the decision to include only the cooling phase by stating: '***... the equilibrium PSC scheme provides a realistic representation of the existence of PSC particles when air temperatures drop below the PSC temperature formation threshold (e.g. Feng et al., 2011). However, the scheme does not represent a slow decline of PSC existence when temperatures rise abruptly above the temperature threshold. Instead PSCs cease to exist instantaneously in the scheme. For this reason only the cooling phase ΔT_{SSO}^{-} of the parameterised temperature fluctuations are coupled to the PSC scheme as the net***

impact on additional PSC formation will be more realistic. Consequently the warm phase is neglected and the net effect on PSC existence might be slightly overestimated.' Secondly, the summary and discussion (section 6) states: '***Our decision to include only the cooling phase implies that this may lead to an overestimate of the impacts of the scheme, and that the diagnosed increase in PSC surface area density should perhaps be considered as an upper bound. Note that consideration of the (neglected) warm phase in the equilibrium PSC scheme would reduce the PSC surface area density change modelled towards the large scale solution obtained in the control integration. By contrast, in a microphysical scheme in which PSC particles are advected around, the particles could briefly exist in air which is above the threshold temperature during the wave-induced warming phase before temperature will fall once again to below the threshold, maintaining PSCs. We simulate this effect by using the cooling phase only.'*** Please see section 6 of the revised manuscript for the full changes made to the summary and discussion section to explain and justify this important point.

2) *"Upstream influence" on PSCs is not mentioned until the summary section of the paper, yet it is apparent in Figure 11 that was presented much earlier. This upstream influence is never adequately explained in the paper. Both Figures 6 and 11 should be plotted without smoothing to make the coarse resolution of the result more clear. This coarse resolution may also influence any discussion of upstream influence, so this should be clarified prior to publication. Suggestions to alleviate this concern appear in context in the section below.*

Firstly, this result is discussed when Figure 11 was introduced. For example, the paragraph in section 5 discussing Figure 11 states that these results are: '***not unexpected ... what is diagnosed in Fig. 11 ... is the difference between two climate equilibrium states ... (which is) locally strongly influenced by the additional parameterisation (adding localised cooling and thus producing more PSCs), but in regions away from the direct impact the response can be determined by feedback mechanisms.***' Nevertheless, we have strengthened mention of the upstream influence of the scheme when Figure 11 is discussed by the inclusion in the revised manuscript of the sentence: '***What is of note here is that PSC differences are occurring both upstream and downstream of the Antarctic Peninsula, i.e. removed from the actual region where the parameterisation acts directly.***'

Secondly, figures 6 and 11 are unsmoothed.

Thirdly, the summary and discussion section clearly states that understanding/clarification of the "upstream influence" on PSCs '***will be the subject of a future study***', as it is beyond the scope of this manuscript.

Detailed Comments:

1) *Page 18292, (Also mentioned on 18288) line 6: "only mountain wave cooling being used" and line 28-29: "The effect of the parameterisation on PSCs is investigated by evaluating the 30 year average difference in PSC surface area density between the perturbation and control". Since parcels would alternately feel both the cooling and heating phases of the orographic wave, this approach needs better*

justification before showing PSC surface area density. The use for evaluating frequency of occurrence of cold T is fine, but here you look at PSC area. The time a parcel experiences the cooling phase will be half of the wave intrinsic (Lagrangian) period. This will be much shorter than the Eulerian period for stationary mountain waves. PSC particles may not grow very large in this time and might sublimate again in the neglected wave warm phase. Hence the method may grossly overestimate PSC area.

We agree with the reviewer and have amended our revised manuscript accordingly. Please see reply to General Comment #1 (above).

2) *18278, 18-21: "increased stratospheric cooling was passed to the PSC scheme of the chemistry–climate model, and caused a 30–50 % increase in PSC surface area density over the Antarctic Peninsula compared to a 30 year control simulation". Including only cooling makes these PSC area statistics a bit dubious. Suggest deleting these specific numbers from the abstract, and instead discussing this more qualitatively. The assumptions do not warrant such a quantitative statement.*

With due respect to the reviewer, we prefer to keep such numbers in the revised manuscript. Please see reply to General Comment #1 (above). However, we reiterate that the numbers are already given broadly (e.g. ***a 30-50% increase***) and always given with the caveat that they are the result of '***increased stratospheric cooling***' only.

3) *18290: Fig 6b would illustrate the point better if plotted without smoothing of the field. This would more clearly show that this is a coarse resolution result. The smoothed field implies the result applies to higher resolution.*

Fig. 6b was not smoothed. This is the actual coarse model result.

4) *18293, 20 and Figure 11: Comment on the mechanism for upstream influence of mountain waves on the PSC area seen in Figure 11 is needed here. How that would come about is certainly not intuitively clear. Is this a model grid/coarse resolution effect? As also suggested for Figure 6, plotting the true coarse resolution result here without smoothing would be clearer and more representative, for example, it may help to understand the upstream influence on PSC area.*

Please see reply to General Comment #2 (above).

5) *18294, 6-8: "This in turn is also a good rational for using the negative temperature anomalies only for the call to the chemistry scheme." Yes, but you further examine geographic distributions of PSC surface area density (e.g. Next page line 9), a quantity that would be greatly affected by processing of air parcels through both cold and warm phases.*

This line has been removed from the revised manuscript.

6) *18294, 9-11: "So for a grid-box averaged PSC coverage only additional incidents below the threshold temperature increase the coverage" Something is wrong with this sentence structure, and the meaning is completely cloudy as a result.*

This line has been removed from the revised manuscript.

7) *18294, 11-12: "Certainly for such an assumption to be true the sub-grid wave train should be slowly evolving horizontally compared to the model time step" This statement mixes space and time scales in a nonsensical way. A method to connect the wave scale to the model time step is with the use of the Lagrangian wave frequency (intrinsic frequency). This can be computed easily from your wave-resolving model simulation for the purpose of this discussion.*

This line has been removed from the revised manuscript.

8) *18294, 29: "due to it representing" "It" here is ambiguous due to a run-on sentence. Suggest splitting this sentence in two.*

Yes, we agree with the reviewer and have split this sentence in two.

9*) 18295, 23: "allow" Do you mean "permits"? The sentence is rather long, but the subject of this verb is apparently "state", so use "permits" or "allows", or rewrite the sentence for clarity.*

Yes, we have followed the reviewers suggestion and changed 'allow' to 'allows'.

10) *18295, 20-21: "The simulation of PSC differences both upstream and downstream of the Antarctic Peninsula" This is the first mention of the upstream influence of the waves on PSCs. It should have been discussed when Figure 11 was introduced. How much of this upstream influence is simply due to the coarse resolution of topography? Perhaps all?*

Please see reply to General Comment #2 (above).

11) *18296, 11-15: "However, future work will investigate replacing the quasi-equilibrium PSC scheme with the full microphysical scheme DLAPSE (Den-itrification by Lagrangian Particle Sedimentation), which uses a Lagrangian trajectory scheme and as such is able to transport PSC particles away from the region of formation" If this is done, representing both warm and cold phases of the gravity waves would become essential.*

Yes. To clearly make this point in the revised manuscript we now (in the summary and discussion section) state: *' **in a microphysical scheme in which PSC particles are advected around, the particles could briefly exist in air which is above the threshold temperature during the wave-induced warming phase before temperature will fall once again to below the threshold, maintaining PSCs. We simulate this effect by using the cooling phase only. In future work we plan is to insert the microphysical scheme DLAPSE (Denitrification by Lagrangian Particle Sedimentation) (Feng et al., 2011) into the UKCA module, and couple it to both the cooling and warming phases of the parameterised temperature fluctuations.**'*

12) *Figure panels are too small in Fig. 2 and 3, but perhaps this will be remedied when the publication appears in full-page format?*

Yes, we will ensure that the typeset manuscript is checked for this, and that all panels are readable in the eventual published manuscript.

Additional author changes

1) We have additionally shown in Figure 6 the cooling phase of the parameterised temperature field. We felt this was required to show the actual field which is coupled to the PSC scheme of the chemistry-climate model. To this end, we have added an additional sentence to section 6, and modified the opening sentence of the first paragraph, so that it now reads: '***Having shown that the parameterised mountain wave-induced temperature fluctuations are broadly consistent with the mesoscale model results, we can progress to assessing the impact of including the wave-induced cooling phase*** ΔT^{-}_{SSO} ***in the chemistry-climate model and coupling it to the PSC scheme, again concentrating on the Antarctic Peninsula. Figure 6 (c) demonstrates the cooling phase*** ΔT^{-}_{SSO} ***for CS3. It is apparent that in this instance it reaches values of around -10 K, which is significantly colder than the cold phases of the overall parameterised temperature fluctuations shown in Fig. 6 (b).***'